

CASAC Ozone Panel: Comments on the Health Risk Assessment

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I am Richard Smith, Professor of Statistics and Biostatistics at the University of North Carolina, and also a consultant to the American Petroleum Institute. The opinions expressed here are my personal views and not the official policy of UNC or API. My comments are addressed primarily at the mortality analyses of Chapter 7, followed by some briefer comments about Chapter 8.

These analyses represent a considerable advance on the first draft HREA, with both the epidemiological and air quality analyses much more comprehensive than before. However, given that all the epidemiology studies find an overall positive association between ozone and mortality, even if it's not uniform, it should not be a surprise that this study finds an overall reduction in mortality when ozone is decreased. To make sense of the numbers, one needs quantitative assessments of uncertainty, and the treatment of that is still incomplete.

In an earlier comment to CASAC, I complained that my own paper (Smith, Xu and Switzer, 2009) had not been cited. This time, it is cited something like 140 times. Never again will I be able to complain to EPA that they never cite my work. However, in fact they have only used a small part of the paper. Specifically, I was asked to provide posterior means and standard deviations of the city-specific ozone-mortality coefficients under both national and regional priors. These analyses are the analog for 8-hour max ozone of the earlier 24-hour ozone results under the national prior of Bell 2004. Other aspects of our paper are not covered, including non-linear modeling of the concentration-response function, spatial dependence of the effect estimates, and the use of city-wide effect modifiers to explain the variation of effect estimates across the country.

Two other epidemiological studies are also included in the mortality analyses. Zanobetti and Schwartz use similar methods to Smith et al., with similar results. However the “long-term” estimates of Jerrett et al. are based on quite different principles and raise a number of sensitivity issues, such as the near-equivalence of the likelihood across a wide range of thresholds and the fact that including a spatial random effect very nearly kills the statistical significance of the association¹. It would be misleading to compare the short-term and long-term mortality results in any direct way.

Next, I would like to discuss the air quality results. In another earlier comment to CASAC I tried to defend the quadratic rollback approach on the grounds of greater transparency, but in the meantime, it is obvious that the EPA researchers have made a great deal of progress with the HDDM method, and I doubt that many people now want to roll back the science. However, I am still concerned about the transparency of this approach. There is an impressive amount of detail in Chapter 4 and its appendices but I would urge EPA to consider publishing raw data from these model runs, so that other researchers can analyze the data using the same or alternative methods. The goal should be to quantify the uncertainty in the mortality reduction estimates taking into account the uncertainties of both the epidemiology and air quality² parts of the analysis. There are still going to be “unknown unknowns” – for instance, it’s still unclear to me how the HDDM approach is supposed to resolve the uncertainties about background ozone – but at least there seems to be a pathway to take account of all the “known unknowns.”

Unfortunately, I don’t think one can say the same about Chapter 8, where an attempt is made to extend the results from 12 cities to the whole US. There are three problems. First, there is no air quality modeling to assess the consequences of a change in the standard – it’s just an assessment of the total number of deaths due to ozone. The second problem follows from the first; in order to do that, you have to be able to estimate the concentration-response function all the way down to zero, and that raises numerous issues³. The third issue is even more fundamental, that despite numerous exhortations in Smith et al (2009) and elsewhere that the national effect estimates cannot be interpreted as if they applied everywhere, the EPA persist in doing that. For this analysis, substituting regional estimates for the national estimate is not a solution – the same issue applies within each region.

In conclusion, the results of chapter 7 are a considerable advance on the first draft REA, but more work is needed to quantify uncertainty when combining the epidemiological and air quality parts of the analysis. The results of Chapter 8 still seem very tentative to me to need a lot of work, and I would question whether they have value in the context of setting a new ozone standard.

Comments added after the meeting

One panel member (I believe it was Dr. Brain) asked me what I would see as the practical agenda, given EPA’s need to prioritize its work. I only gave a very brief response at the meeting but would like to expand a little now.

Of course I recognize the limited time frame for the immediate discussion but I view my comments in the context of what I see as a developing research exercise. I actually feel the results would be stronger if EPA could provide realistic uncertainty quantification for its risk estimates, in order to show that the projected mortality reductions are not just noise in the simulations. I do not know how the results would come out, but I think it would be a very interesting exercise.

Specifically, what I am calling for is an attempt to assess the uncertainty in the HDDM models, that would complement the uncertainty estimates already available for the epidemiological models. However, I would also point out that simply asking for 95% confidence intervals (or posterior credible intervals) on the epidemiological coefficients, and then carrying those through into the risk estimates, is a very incomplete way of handling the uncertainty in the epidemiological estimates. Full posterior distributions (in the form of Monte Carlo samples) are available or could easily be generated, and provide a more satisfactory way of integrating uncertainties in the epidemiological coefficients with other sources of uncertainty or randomness. Of the alternative epidemiological estimates I mentioned, I would particularly highlight the case for including nonlinear (spline-based or piecewise linear) estimates as a complement to the log-linear C-R functions. One of the arguments sometimes presented against using nonlinear C-R curves is that the results are hard to interpret: it is much easier to quote a single coefficient (10 ppb rise in ozone increases the mortality rate by X%) than to explain and interpret a whole curve. However in the context of a risk assessment that is less of an issue, because the end result of the calculation is a single number (the projected mortality reduction under a specified scenario of emissions reduction). The calculation itself is more complicated using a nonlinear C-R curve, but the end result need not be.

As for the national estimates, my feeling is this needs a rethink of the basic objectives. None of the three main epidemiological studies cited in the HREA was based on a random nationally based sample, so there really is not any basis for claiming that they are representative. However in the case of NMMAPS, the 98 cities in the database cover about 40% of the US population (based on the population numbers used when the database was assembled) and it should be possible to calculate the cumulative risk across all 98 cities. That seems to me the most reasonable basis for discussion of a national standard.

In terms of the timescale, I am aware that the EPA is under a court-imposed deadline and that the kind of revisions I am suggesting go well beyond what is reasonable to expect between the second and third drafts of this HREA. But the ideas of a national risk assessment (for PM as well as ozone) will persist well beyond the short time frame of the present discussion, and the expertise needed is available in the academic community even if it does not all exist in-house at the EPA. From the point of view of providing better informed estimates for the future, I don't think what I am proposing is unrealistic.

Endnotes

1. According to Table 3S of Jerrett et al. (2009, online supplement), -2 log likelihood for the threshold model is 143758.93 at threshold 0, 143755.39 at threshold 56 (the maximum likelihood estimate), 143759.48 at threshold 60. According to a standard interpretation of likelihood ratios, any threshold for which -2 log likelihood is less than 143759.23 (the minimum plus 3.84, which is the 95% point of the χ^2 distribution with 1 DF) is in the 95% confidence interval. That includes everything from a threshold 0 to just below 60.

The statistical interpretation is that all these thresholds are consistent with the data. As shown by Anne Smith in another presentation at the CASAC meeting, these thresholds have dramatically different interpretations for the projected mortality reductions. As for random spatial effects, for three of the alternative risk estimates present in Table 2S of Jerrett et al. (2009, online supplement), the lower bound of the 95% CI on the risk ratio is either 1.001 or 1.002, implying borderline statistical significance. The practical interpretation of this is that, while no model is uniformly identified as “best”, there are statistical models consistent with the data for which the statistical significance of the effect is in question.

2. For example, the HREA around page 4-16 described a number of supplementary regression analyses used to examine sensitivities in the HDDM analyses. As far as I can tell, the uncertainties in those regressions are not considered.
3. Bell et al. (2006) presented national average estimates (with confidence bands) for a nonlinear spline-based C-R curve based on 24-hour ozone. Similar results were calculated by the present author for the 8-hour maximum ozone metric, but were not included in Smith et al. (2009) because of a difficulty in reconciling the results with the earlier calculations by Bell. However, Smith et al. (2009) did present results based on an alternative piecewise linear approach. For all of these methods, there is a difficulty in validating the C-R function all the way down to 0 because there is very little data for ozone values below about 20 ppb.

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